Should any one be interested in the subject, I would refer them to the experiments of Mr. J. T. Bottomley (*Proc. R. Soc.*, No. 197, 1879), of Prof. Ewing (*Proc. R. Soc.*, 1880, June 10), and of myself (" Influence of Stress and Strain on the Action of Physical Forces, *Phil. Trans.*, 1882, second volume).

HERBERT TOMLINSON

King's College, Strand, December 4

Intra-Mercurial Planets—Prof. Stewart's 24·011d. Period, Leverrier's and Gaillot's 24·25d., and Leverrier's 33·0225d. Sidereal Periods Considered

As your regular monthly numbers did not reach our Free Library from September, 1881, until comparatively recently, and I was absent from home when they did arrive, it was only quite lately that I had an opportunity of seeing Prof. Balfour Stewart's very interesting paper "On the Possibility of Intra-Mercurial Planets," read at last year's meeting of the British Association, and published at length in your issue of September 15, 1881. "The possibility" has been almost an admitted fact for over a century, but Prof. Stewart's valuable paper discusses the relation of certain sun-spot periods to a probable sidereal period, approximately at least, of an intra-Mercurial planet of 24·011 days.

On looking through your subsequent numbers, I was rather surprised that so suggestive a paper had not elicited quite a discussion, although it is true that Prof. Stewart remarked that " the test was not yet complete," and many may have waited to see the final results, which have not yet appeared, but perhaps will be forthcoming at the next meeting of the British Association. But the first point that struck me, although not referred to by Prof. Stewart, was the near approximation periods of 24·011 days affords to Leverrier's and Gaillot's period of 24·25 days noticed in your columns of August 22, 1878, which M. Gaillot endeavoured to fit to Prof. Watson's observation, in Wyoming State, of a supposed intra-Mercurial planet at 2° 9' from the Sun, during *the total eclipse*, July 29, 1878. M. Gaillot's difficulty seemed to be to reconcile Leverrier's formula with Prof. Watson's reasonable belief that he saw the planet in the superior part of the orbit, while Gaillot made the formula and interval require it to be in the inferior part of the orbit July 29, 1878. The only interval that Gaillot referred to was from 1750 (January 1, I presume) ; it might have been obvious, therefore, that quite a small fractional difference in each of so many revolutions would suffice to make the period accord with either condition that Prof. Watson's observation required ; namely, *that the planet was seen at 2° 9' from superior conjunction,* or 2°·9' *past inferior conjunction.* For instance, I have obtained these two result for the synodical periods. The same interval for both about 46962½ days, requiring 1808$\frac{11}{24}$ revolutions of 25·96825104d., each $\frac{11}{24}$ being equal to 11·90211506d. ; that accords very closely with Prof. Watson's belief, while 1808$\frac{1}{24}$ revolutions of 25·9742355d. each, and 1·08226d. remainder, meet the condition of its being 2° 9' past the inferior conjunction. Of course, as a matter of opinion, I presume it would be impossible to see the planet so near its inferior conjunction during any total eclipse of the Sun, the planet's crescent being altogether too fine. These results are simply what the conditions require in relation to approximate 26-day apparent-periods, but we must avoid exactly 26 days, or the interval would put the planet at *its elongation*, perhaps apparently 10° from the Sun, and if we tried the Lescarbault interval, from March 26, 1859, 7065½ days, singularly enough it would put the planet *in the other elongation*. Fractional differences are of course very important therefore. And I do not find either that M. Gaillot's figures 24·25d. for the sidereal time, and 14°·8462 for the diurnal motion exactly accord, and neither fills the conditions required by Prof. Watson's observation, if I am approximately correct, which I think I am. For instance, 14°·8462 diurnal motion, gives us 24·24862928d. for the sidereal periods, not 24·25d., and the synodical period would be 25·9729903466d., and the planet's position would be about 46° 12' in its orbit past inferior conjunction, or apparently about 8° 24' from the Sun, and 46° 12' would be about 3⅓ days.

The sidereal period of 24·25 days, makes the diurnal motion 14°·8453608247, and puts the planet at about 6°·8 past inferior conjunction, or apparently less than 1°. The synodical revolutions would be 25·974562624d. and fractional remainder ·0188945, or 11h. 46m. 43s., which of course would be too close to the sun. But the sidereal period and the diurnal motion should both agree, instead of producing such a difference as I have here

indicated, of nearly 40° in the revolutions. But although I believe we cannot accept the exact published figures, 24·25d., or 14°·8462, still I have shown how near we may make the final results conform to them.

Adopting the same number of synodical revolutions, and practically making the best use of the formula, obtaining 1808$\frac{11}{24}$ revolutions, and 1808$\frac{1}{24}$. The revolution being 25·96825104d., or 25·9742355d., and the remainders 11·90211506d., or 1·08226. Reduced to clock time they stand as follows : 1808$\frac{11}{24}$ being equal to 25d. 23h. 14m. 16·9s. each, and 11d. 23h. 39m. 2·7s. remainder, and 1808$\frac{1}{24}$ being equal to 25d. 23h. 22m. 54s. each, and 1d. 1h. 58m. 27$\frac{1}{4}$s. remainder. *The latter is almost absolutely identical* with the periods that would fit the Fritsch and Stark interval from October 10, 1802, to October 9, 1819, 6208 days, or 239 periods of 25d. 23h. 23m. 51s. And from Stark to Lescarbault makes 14,413 days, which would require 555 periods of 25d. 23h. 15m. 53½s., *which affords almost exact identity* with the general mean, placing Prof. Watson's observation in the superior part of the orbit. Thus, then, we have almost positive assurance that Fritsch, Stark, Lescarbault, and Prof. Watson's planet were identical, and that Prof. Watson was correct about it being 2° 9' from superior conjunction : these interesting facts, giving a record to Lescarbault's planet of 80 years from Fritsch's observance October 10, 1802, to October next. What other " myths " will stand such satisfactory results ? I am afraid that Prof. Proctor and some other astronomers have not given the attention to this question that it deserves. But there are a few exceptions deserving credit : M. de la Baume, in Paris, was engaged last year in a classification of reported observed transits, although he did not then draw any inferences respecting apparent revolutions. He regarded Fritsch, De Cuppe, Lescarbault, and Lummis' transits as the same planets, agreeing relatively with the nodes. While Lichtenburg, November 19, 1762, Hoffman, about May 10, 1764, Scott, June 28, 1847, Ritter and Schmidt, June 11, 1855, and W. G. Wright, of San Bernardino, California, October 24, 1876, whose transit was illustrated in the *Scientific American* of November 18, 1876, he regarded as another larger planet than Lescarbault's.

Adopting the same principle with Prof. Stewart's hypothetical sidereal periods of 24·011d., I first find what results that gives, as applied to the same interval from January 1, 1750, and then take the nearest modification I can to the conditions of Prof. Watson's observation. 360° divided by 24·011d. gives us 14°·99312818 for the planet's diurnal motion, which, multiplied by 46,962½ days, gives us 704114°·78215325, from which, subtracting the earth's motion, 46288°·463941, leaves a residue of 657826°·318213, which, divided by 360° gives us 1827·29533 synodical revolutions ; using that to divide the 46962½ days, we obtain the synodical periods of 25·700553d. The fractional revolution ·29533 is equal to 106° 19' 17", or 7d. 14h. 10m. Now while that would put the planet in the superior part of the orbit, it would still be nearly 60° from where Prof. Watson observed it. I ought to have explained before that 2° 9', or 2° 10' apparently, is about equivalent to 15° from *superior* conjunction, or, 15° past *inferior* conjunction in the planet's orbit ; 15° from 180°, therefore, leaves 165° as the required position, instead of 106° 19' 17". Perhaps I am only approximately correct, but sufficient for illustration. It is very evident, however, that a very slight modification of Prof. Stewart's inferential sidereal periods, 24·011d., would give us the 60° more required, or exact accord with Prof. Watson's observation, and the evidence would be rather in favour of 1827$\frac{11}{24}$ revolutions, obtained from such a solar analogy, and may still have an incidental bearing or relation to the Leverrier-Gaillot formula I have construed to require 1808$\frac{11}{24}$ or 1808$\frac{1}{24}$ revolutions. 1827$\frac{11}{24}$ revolutions would give us 25·698260334253d. for the synodical periods, and a remainder of 11·77836931986d. Reduced to clock time, that would give us 25d. 16h. 45m. 29·7s. for each apparent revolution, and 11d. 18h. 40m. 51·11s. for the remainder. It must be understood that these definitions, 1808$\frac{11}{24}$ and 1827$\frac{11}{24}$, with their results, are intended only to express possible general mean periods of apparent revolutions, and may not exactly apply to any of the intervals between the long list of recorded observations of supposed transits. When Leverrier, October 1876, had strong faith in sidereal periods of 33·0225d., it was probably a general mean from January 1, 1750, to Lescarbault, March 26, 1859, and only approximately fitted Lummis, De Cuppis, Stark, Fritsch, and others, but still in a general sense applied to some of them, while Leverrier was led to predi-

cate a transit, March 22, 1877, which probably did not occur. And yet, viewed in connection with a reported transit, October 24, 1876, seen by Mr. W. G. Wright at San Bernardino, California, and illustrated and fully reported in the *Scientific American* of November 18, 1876, which circumstance Leverrier probably had no knowledge of, was by no means as unsatisfactory as the public imagined ; for practically *Wright's transit and Leverrier's hypothetical period mutually confirmed each other.* The *Scientific American* Supplement of August 27, 1881, published some remarks I sent them, which may have reached England. One point I directed attention to was that Leverrier indicated that a conjunction was due September 21, 1876, and I found that there were thirty-three days between that and October 24, 1876, so if Leverrier took 176 synodical periods from Lescarbault to September 21, it was nearly the same thing to take 177 to October 24 ; but extending the interval to January 1, 1750, there would be much nearer similarity in the synodical periods to accord with Wright's transit, October 24, 1876. I also noticed that the ratio of displacement of the node from Lescarbault and Lummis was 7 days retrograde in 3 years' advance, and on that data, applied to Wright's transit, another transit would be due 11⅔ days earlier in 1881, while Leverrier, in October, 1876, remarked that *for a transit at this node we must wait till about* 1881." My computation made it fall due, therefore, October 12 or 13, 1881, and I was anxious that it might be looked for. The computation made the Hawaiian Islands the most favourable place ; but although I believe it was not seen there, nor was it observed from Sacramento or Salt Lake City, where Mr. W. R. Frink looked for it with a 4-inch aperture achromatic telescope, we have no evidence to show whether it might not have occurred in Europe or elsewhere, and been noticed if it had been looked for.

Sacramento, California   A. F. GODDARD

[The subject of this communication is a very interesting one, as relating to the possibility of changes on the sun's surface being due in some way to the positions of the various planets of the system. But before this relation can be considered as established, it will be necessary to increase the accuracy of our solar information by collecting our past observations, as well as by securing a set of daily observations for the future.—ED.]

An Extraordinary Meteor

I BEG to send you the following, in case you consider it worth inserting :—At about 1.10 a.m. on the night between November 18 and 19, whilst going in the s.s. *Bokhara* in the Red Sea, about midway from Aden to Suez, the quarter-master on duty called me, saying he had just seen a new comet, or shooting star, which was still visible many minutes after its first appearance. He said that whilst he was looking out ahead, or in a northerly direction, he suddenly noticed the effect of a bright light shining from astern, and on turning round saw a very bright shooting star still moving from left to right, and slightly downwards, in the south, at an altitude of about 40°. The star speedily disappeared, but left a bright train of light behind it, which continued so long (from five to ten minutes he guessed) that he thought I might like to see it. I came on deck a little before a quarter past one by the ship's clock, and found a streak of light which I estimated as 8° or 10° in length, and rather less than half a degree in width, apparently stationary, midway between Sirius and Canopus, and nearly as bright as *the* comet, the head of which must have risen half an hour more previously. I watched the streak till half-past one o'clock, when it seemed sensibly fainter, though still a conspicuous object, notwithstanding the presence of the moon, the comet, and a number of bright stars. Whilst watching I noticed two small meteors shooting from left to right across the southern sky, which struck me as probably belonging to the same group as the large one whose train I was watching.

At half-past one o'clock I went below, and did not return on deck till 5 o'clock, when the apparition had disappeared. The quarter-master told me afterwards that it had faded away soon after I left the deck, but he believed that from first to last it had remained conspicuously bright for more than half an hour.

Clewer, December 6   B. R. BRANFILL

British Rainfall

I AM just preparing to issue to all the observers of rainfall known to me, blank forms for the entry of their records for the year shortly about to close. This staff now exceeds 2000, but still as they are not unfrequently rather clustered there are many parts of the country where additional records are needed. I have no doubt that records are already kept in many places unknown to me, and I shall be glad if you will allow me to invite communications from any one who has kept an accurate record, and to supply either those already observing or contemplating doing so with a copy of the rules adopted by British observers, and with all necessary blank forms—all, I may perhaps as well add, free of charge, as our greatest requirements are *ample* and *accurate* records.   G. J. SYMONS

62, Camden Square, London, N.W.

Swan Lamp Spectrum and the Aurora

IN NATURE, vol. xxv. p. 347, is a description of the spectrum of carbon as found by Professors Liveing and Dewar in a Swan lamp rendered incandescent in the ordinary way. Finding one of these lamps only feebly lighted by ten pint Grove cells, it occurred to me to test it by the secondary current. The coil was nominally a 6-inch spark one, but little battery power was used, and the spark considerably reduced. One wire was connected with the filament holders, the one made into a little coil and laid on the top of the lamp.

The first effect was a fine silver glow filling the lamp, and showing Plücker tube changes when the circuit was reversed. This gave a carbon spectrum of bright lines. Soon, however, the colour of the discharge changed to pink, and the carbon spectrum gave way to a nitrogen banded one. A yellow spark had been noticed where the wire lay on the top of the lamp, and it was evident air had found its way into it.

At one point perforation had taken place by a single spark, while near this the glass was pounded into a sponge-like mass by a series of these.

The sodium lines due to disintegration of the glass were observed in the spark and glow. I was much struck by the rapidity with which, as what was probably only a small quantity of air found its way into the lamp, the nitrogen-spectrum swept away and took the place of the carbon one, a matter which seems to present another difficulty to the favourite theory which makes the aurora, with its bright, sharp unrecognized lines, an electric discharge in rarified air.   J. RAND CAPRON

Guildown, November 30

The Aurora

ALREADY we have for the height of the "auroral beam" the varying estimates of 44, 170, 200, and 212 miles, and assuming the correctness of any one of the three last figures, we seem drifting from the improbable to the impossible, for are we not told by Messrs. De la Rue and Müller (NATURE, vol. xxii. p. 24) that while at 81·47 miles' height, the discharge is "pale and faint, at 124·15" *no discharge could pass?* Lest this addition to the aurora's mysteries be for want of definite particulars in the observations, I add mine as nearly as I can—Time 6h. G.M.T. and a few minutes? Altitude of moon above horizon 28°. Distance from moon's centre to centre of beam as it floated *above* 2°, direction east to west (nearly). Lat. 51° 13' 46" N., Long. 0° 28' 47" W. (observatory).

Guildown, Guildford, December 11   J. RAND CAPRON

Fertilisation of the Speedwell

IF Mr. Stapley, who wrote on this subject in last week's NATURE, can refer to Dr. H. Müller's treatise on the relations between flowers and insects, in the first volume of Shenk's *Handbuch der Botanik* (now publishing as part of Trewendt's *Encyclopædie der Naturwissenschaften*), he will see that his own observations are very similar to those of Dr. Müller. The latter, however, refers to and figures the Germander, not the Common, Speedwell. Is it possible that Mr. Stapley—who speaks of the *Veronica officinalis* as having larger flowers than the *V. hederæfolia*, whereas they have flowers of about the same size—mistakes the *V. chamædrys* for the *V. officinalis?*

The insects which Dr. Müller found bending down the stamens, as Mr. Stapley describes, were small Diptera chiefly of the genera *Ascia* and *Melanostoma*. He mentions this also in *Kosmos*, iii. p. 497, and a few pages earlier (*ib.* p. 493) he gives a large drawing of *V. urticæfolia.*